THE GENESIS OF SPECIES.

CHAPTER V.

AS TO SPECIFIC STABILITY

What is meant by the Phrase "Specific Stability;" such Stability to be expected *a priori*, or else Considerable Changes at once. - Rapidly-increasing Difficulty of intensifying Race Characters; Alleged Causes of this Phenomenon; probably an Internal Cause coöperates. - A Certain Definiteness in Variations. - Mr. Darwin admits the Principle of Specific Stability in Certain Cases of Unequal Variability. - The Goose. - The Peacock. - The Guinea-fowl. - Exceptional Causes of Variation under Domestication. - Alleged Tendency to Reversion. - Instances. - Sterility of Hybrids. - Prepotency of Pollen of same Species, but of Different Race. - Mortality in Young Gallinaceous Hybrids. - A Bar to Intermixture exists somewhere. - Guinea-pigs. - Summary and Conclusion.

As was observed in the preceding chapters, arguments may yet be advanced in favor of the opinion that species are stable (at least in the intervals of their comparatively sudden successive manifestations) ; that the organic world consists, according to Mr. Galton's before-mentioned conception, of many faceted spheroids, each of which can repose upon any one facet, but, when too much disturbed, rolls over till it finds repose in stable equilibrium upon another and distinct facet. Something, it is here contended, may be urged, in favor of the existence of such facets - of such intermitting conditions of stable equilibrium.

A view as to the stability of species, in the intervals of change, has been well expressed in an able article, before quoted from, as follows: [1] "A given animal or plant appears to be contained, as it were, within a sphere of variation:

[1] "*North British Review*, New Series vol. vii., March, 1867, p. 282.

one individual lies near one portion of the surface; another individual, of the same species, near another part of the surface; the average animal at the centre. Any individual may produce descendants varying in any direction, but is more likely to produce descendants varying toward the centre of the sphere, and the variations in that direction will be greater in amount than the variations toward the surface." This might be taken as the representation of the normal condition of species (i. e., during the periods of repose of the several facets of the spheroids), on that view which, as before said, may yet be defended.

Judging the organic world from the inorganic, we might expect, *a priori*, that each species of the former, like crystallized species, would have an approximate limit of form, and even of size, and at the same time that the organic, like the inorganic forms, would present modifications in correspondence with surrounding conditions; but that these modifications would be, not minute and insignificant, but definite and appreciable, equivalent to the shifting of the spheroid on to another facet for support.

Mr. Murphy says, [2] "Crystalline formation is also dependent in a very remarkable way on the medium in which it takes place." "Beudant has found that common salt, crystallizing from pure water, forms cubes; but if the water contains a little boracic acid, the angles of the cubes are truncated. And the Rev. E. Craig has found that carbonate of copper, crystallizing from a solution containing sulphuric acid, forms hexagonal tubular prisms; but if a little ammonia is added, the form changes to that of a long, rectangular prism, with secondary planes in the angles. If a little more ammonia is added, several varieties of rhombic octahedra appear; if a little nitric acid is added, the rectangular prism appears again. The changes take place not by the addition of new crystals, but by changing the growth

[2] "Habit and Intelligence," vol. i., p. 75.

of the original ones." These, however, may be said to be the same species, after all; but recent researches by Dr. H. Charlton Bastian seem to show that modifications in the conditions may result in the evolution of forms so diverse as to constitute different organic species.

Mr. Murphy observes [3] that "it is scarcely possible to doubt that the various forms of fungi which are characteristic of particular situations are not really distinct species, but that the same germ will develop into different forms, according to the soil on which it falls;" but it is possible to interpret the facts differently, and it may be that these are the manifestations of really different and distinct species, developed according to the different and distinct circumstances in which each is placed. Mr. Murphy quotes Dr. Carpenter [4] to the effect that "no *Puccinia* but the *Puccinia rosæ* is found upon rose-bushes, and this is seen nowhere else; *Omygena exigua* is said to be never seen but on the hoof of a dead horse; and *Isaria felina* has only been observed upon the dung of cats, deposited in humid and obscure situations." He adds, "We can scarcely believe that the air is full of the germs of distinct species of fungi, of which one never vegetates until it falls on the hoof of a dead horse, and another, till it falls on cat's dung in a damp and dark place." This is true, but it does not quite follow that they are necessarily the same species, if, as Dr. Bastian seems to show, thoroughly different and distinct organic forms [5] can be evolved one from another by modifying the conditions. This observer has brought forward arguments and facts from which it would appear that such definite, sudden, and considerable transformations may take place in the lowest organisms. If such is really the case, we might expect, *a priori*, to find in the

[3] "Habit and Intelligence," vol. i., p. 202.

[4] "Comparative Physiology," p. 214, note.

[5] See *Nature*, June and July, 1870, Nos. 35, 36, 37, pp. 170, 193, 219.

highest organisms a tendency (much more impeded and rare in its manifestations) to similarly appreciable and sudden changes, under certain stimuli; but a tendency to continued stability, under normal and ordinary conditions. The proposition that species have, under ordinary circumstances, a definite limit to their variability, is largely supported by facts brought forward by the zealous industry of Mr. Darwin himself. It is unquestionable that the degrees of variation which have been arrived at in domestic animals have been obtained more or less readily in a moderate amount of time, but that further development in certain desired directions is in some a matter of extreme difficulty, and in others appears to be all but, if not quite, an impossibility. It is also unquestionable that the degree of divergence which has been attained in one domestic species is no criterion of the amount of divergence which has been attained in another. It is contended on the other side that we have no evidence of any limits to variation other than those imposed by physical conditions, such, e. g., as those which determine the greatest degree of speed possible to any animal (of a given size) moving over the earth's surface; also it is said that the differences in degree of change shown by different domestic animals depend in great measure upon the abundance or scarcity of individuals subjected to man's selection, together with the varying direction and amount of his attention in different cases; finally, it is said that the changes found in Nature are within the limits to which the variation of domestic animals extends - it being the case, that when changes of a certain amount have occurred to a species under nature, it becomes *another species*, or sometimes *two or more other species* by divergent variations, each of these species being able again to vary and diverge in any useful direction.

But the fact of the rapidly-increasing difficulty found in producing, by ever such careful selection, any further extreme in some change already carried very far (such as the tail of the "fantailed pigeon," or the crop of the "pouter"), is certainly, so far as it goes, on the side of the existence of definite limits to variability. It is asserted, in reply, that physiological conditions of health and life may bar any, such further development. Thus, Mr. Wallace says [6] of these developments: "Variation seems to have reached its limits in these birds. But so it has in nature. The fantail has not only more tail-feathers than any of the three hundred and forty existing species of pigeons, but more than any of the eight thousand known species of birds. There is, of course, some limit to the number of feathers of which a tail useful for flight can consist, and in the fantail we have probably reached that limit. Many birds have "the esophagus, or the skin of the neck, more or less dilatable, but in no known bird is it so dilatable as in the pouter pigeon. Here again the possible limit, compatible with a healthy existence, has probably been reached. In like manner, the difference in the size and form of the beak in the various breeds of the domestic pigeon, is greater than that between the extreme forms of beak in the various genera and sub-families of the whole pigeon tribe. From these facts, and many others of the same nature, we may fairly infer that, if rigid selection were applied to any organ, we could, in a comparatively short time, produce a much greater amount of change than that which occurs between species and species in a state of nature, since the differences which we do produce are often comparable with those which exist between distinct genera or distinct families."

But, in a domestic bird like the fantail, where Natural Selection does not come into play, the tail-feathers could hardly be limited by "utility for flight," yet two more tailfeathers could certainly exist in a fancy breed, if "utility for flight" were the only obstacle. It seems probable that the

[6] "Natural Selection," p. 293.

real barrier is an *internal* one in the nature of the organism, and the existence of such is just what is contended for in this chapter. As to the differences between domestic races being greater than those between species, or even genera, that is not enough for the argument. For, upon the theory of "Natural Selection" all birds have a common origin, from which they diverged by infinitesimal changes, so that we ought to meet with sufficient changes to warrant the belief that a hornbill could be produced from a hummingbird, proportionate time being allowed.

But not only does it appear that there are barriers which oppose change in certain directions, but that there are positive tendencies to development along certain special lines. In a bird which has been kept and studied like the pigeon, it is difficult to believe that any remarkable spontaneous variations would pass unnoticed by breeders, or that they would fail to be attended to and developed by some one fancier or other. On the hypothesis of *indefinite* variability, it is then hard to say why pigeons with bills like toucans, or with certain feathers lengthened like those of trogans, or those of birds of paradise, have never been produced. This, however, is a question which may be settled by experiment. Let a pigeon be bred with a bill like a toucan's, and with the two middle tail-feathers lengthened like those of the king-bird of paradise, or even let individuals be produced which exhibit any marked tendency of the kind, and indefinite variability shall be at once conceded.

As yet, all the changes which have taken place in pigeons are of a few definite kinds only, such as may be well conceived to be compatible with a species possessed of a certain inherent capacity for considerable yet definite variation, a capacity for the ready production of certain degrees of abnormality, which then cannot be further increased.

Mr. Darwin himself has already acquiesced in the proposition here maintained, inasmuch as he distinctly affirms the existence of a marked internal barrier to change in certain cases. And if this is admitted in one case, the *principle* is conceded, and it immediately becomes probable that such internal barriers exist in all, although enclosing a much larger field for variation in some cases than in others. Mr. Darwin abundantly demonstrates the variability of dogs, horses, fowls, and pigeons, but he none the less shows clearly the *very small* extent to which the goose, the peacock, and the guinea-fowl have varied. [7] Mr. Darwin attempts to explain this fact as regards the goose by the animal being valued only for food and feathers, and from no pleasure having been felt in it on other accounts. He adds, however, at the end the striking remark, [8] which concedes the whole position, "but the goose seems to have a *singularly inflexible organization*." This is not the only place in which such expressions are used. He elsewhere makes use of phrases which quite harmonize with the conception of a normal specific constancy, but varying greatly and suddenly at intervals. Thus he speaks [9] of a *whole organization seeming to have become plastic, and tending to depart from the parental type*. That different organisms should have different degrees of variability, is only what might have been expected *a priori* from the existence of parallel differences in inorganic species, some of these having but a single form, and others being polymorphic.

To return to the goose, however, it may be remarked that it is at least as probable that its fixity of character is the cause of the neglect, as the reverse. It is by no means unfair to assume that *had* the goose shown a tendency to vary similar in degree to the tendency to variation of the

[7] "Animals and Plants under Domestication," vol. i., pp. 289-295.

[8] "Origin of Species," 5th edit., 1869, p. 45.

[9] Ibid., p. 13.

fowl or pigeon, it would have received attention at once on that account.

As to the peacock it is excused on the pleas (1), that the individuals maintained are so few in number, and (2) that its beauty is so great it can hardly be improved. But the individuals maintained have not been too few for the independent origin of the black-shouldered form, or for the supplanting of the commoner one by it. As to any neglect in selection, it can hardly be imagined that with regard to this bird (kept as it is all but exclusively for its beauty), any spontaneous beautiful variation in color or form would have been neglected. On the contrary, it would have been seized upon with avidity and preserved with anxious care. Yet apart from the black-shouldered and white varieties, no tendency to change has been known to show itself. As to its being too beautiful for improvement, that is a proposition which can hardly be maintained. Many consider the Javan bird as much handsomer than the common peacock, and it would be easy to suggest a score of improvements as regards either species.

The guinea-fowl is excused, as being "no general favorite, and scarcely more common than the peacock;" but Mr. Darwin himself shows and admits that it is a noteworthy instance of constancy under very varied conditions.

These instances alone (and there are yet others) seem sufficient to establish the assertion that degree of change is different in different domestic animals. It is, then, somewhat unwarrantable in any Darwinian to assume that *all* wild animals have a capacity for change similar to that existing in *some* of the domestic ones. It seems more reasonable to assert the opposite, namely, that if, as Mr. Darwin says, the capacity for change is different in different domestic animals, it must surely be limited in those which have it least, and *a fortiori* limited in wild animals.

Indeed, it cannot be reasonably maintained that wild species certainly vary as much as do domestic races; it is possible that they may do so, but at least this has not been yet shown. Indeed, the much greater degree of variation among domestic animals than among wild ones is asserted over and over again by Mr. Darwin, and his assertions are supported by an overwhelming mass of facts and instances.

Of course it may be asserted that a tendency to indefinite change exists in all cases, and that it is only the circumstances and conditions of life which modify the effects of this tendency to change so as to produce such different results in different cases. But assertion is not proof, and this assertion has not been proved. Indeed, it may be equally asserted (and the statement is more consonant with some of the facts given), that domestication in certain animals induces and occasions a capacity for change which is wanting in wild animals - the introduction of new causes occasioning new effects. For, though a certain degree of variability (normally, in all probability, only oscillation) exists in all organisms, yet domestic ones are exposed to new and different causes of variability, resulting in such striking divergencies as have been observed. Not even in this latter case, however, is it necessary to believe that the variability is indefinite, but only that the small oscillations become in certain instances intensified into large and conspicuous ones. Moreover, it is possible that some of our domestic animals have been in part chosen and domesticated through possessing variability in an eminent degree.

That each species exhibits certain oscillations of structure is admitted on all hands. Mr. Darwin asserts that this is the exhibition of a tendency to vary which is absolutely indefinite. If this indefinite variability *does* exist, of course no more need he said. But we have seen that there are arguments *a priori* and *a posteriori* against it, while the occurrence of variations in certain domestic animals greater in degree than the differences between many wild species, is no argument in favor of its existence, until it can be shown that the causes of variability in the one case are the same as in the other. An argument against it, however, may be drawn from the fact that certain animals, though placed under the influence of those exceptional causes of variation to which domestic animals are subject, have yet never been known to vary, even in a degree equal to that in which certain wild kinds have been ascertained to vary.

In addition to this immutability of character in some animals, it is undeniable that domestic varieties have little stability, and much tendency to reversion, whatever be the true explanation of such phenomena.

In controverting the generally received opinion as to "reversion," Mr. Darwin has shown that it is not all breeds which in a few years revert to the original form; but he has shown no more. Thus, the feral rabbits of Porto Santo, Jamaica, and the Falkland Islands, have not yet so reverted in those several localities. [10] Nevertheless, a Porto Santo rabbit brought to England reverted in a manner the most striking, recovering the proper color of its fur "in rather less than four years." [11] Again, the white silk fowl, in our climate, "reverts to the ordinary color of the common fowl in its skin and bones, due care having been taken to prevent any cross." [12] This reversion taking place in spite of careful selection, is very remarkable.

Numerous other instances of reversion are given by Mr. Darwin, both as regards plants and animals; among others, the singular fact of bud reversion.[13] The curiously-recurrring development of black sheep, in spite of the most careful breeding, may also be mentioned, though, perhaps, reversion has no part in the phenomenon.

These facts seem certainly to tell in favor of limited

[10] "Animals and Plants under Domestication," vol. i., p. 115.
[11] Ibid., vol. i., p. 114. [12] Ibid., vol. i., p. 243.
[13] Ibid., vol. ii., p. 361.

variability, while the cases of non-reversion do not contradict it, as it is not contended that all species have the same tendency to revert, but rather that their capacities in this respect, as well as for change, are different in different kinds, so that often reversion may only show itself at the end of very long periods indeed.

Yet some of the instances given as probable or possible causes of reversion by Mr. Darwin, can hardly be such. He cites, for example, the occasional presence of supernumerary digits in man. [14] For this notion, however, he is not responsible, as he rests his remark on the authority of a passage published by Prof. Owen. Again, he refers [15] to "the greater frequency of a monster proboscis in the pig than in any other animal." But with the exception of the peculiar muzzle of the Saiga (or European antelope), the only known proboscidian Ungulates are the elephants and tapirs, and to neither of these has the pig any close affinity. It is rather in the horse than in the pig that we might look for the appearance of a reversionary proboscis, as both the elephants and the tapirs have the toes of the hind-foot of an odd number. It is true that the elephants are generally considered to form a group apart from both the odd and the even toed Ungulata. But of the two, their affinities with the odd-toed division are more marked. [16]

Another argument in favor of the, at least intermitting, constancy of specific forms and of sudden modification, may be drawn from the absence of minute transitional forms, but this will be considered in the next chapter.

[14] "Animals and Plants under Domestication," vol. ii., p. 16.

[15] Ibid., vol. ii., p. 57.

[16] This has been shown by my late friend Mr. H. N. Turner, Jr., in an excellent paper by him in the "Proceedings of the Zoological Society for 1849," p. 147. The untimely death, through a dissecting wound, of this most promising young naturalist, was a very great loss to zoological science.

It remains now to notice in favor of specific stability, that the objection drawn from physiological difference between "species" and "races" still exists unrefuted.

Mr. Darwin freely admits difficulties regarding the sterility of different species when crossed, and shows satisfactorily that it could never have arisen from the action of "Natural Selection." He remarks [17] also: "With some few exceptions, in the case of plants, domesticated varieties, such as those of the dog, fowl, pigeon, several fruit-trees, and culinary vegetables, which differ from each other in external characters more than many species, are perfectly fertile when crossed, or even fertile in excess, while closely allied species are almost invariably in some degree sterile."

Again, after speaking of "the general law of good being derived from the intercrossing of distinct individuals of the same species," and the evidence of the pollen of a distinct *variety* or race is prepotent over a flower's own pollen, adds the very significant remark, [18] When distinct *species* are crossed, the case is directly the reverse, for a plant's own pollen is almost always prepotent over foreign pollen."

Again he adds: [19] "I believe from observations communicated to me by Mr. Hewitt, who has had great experience in hybridizing pheasants and fowls, that the early death of the embryo is a very frequent cause of sterility in first crosses. Mr. Salter has recently given the results of an examination of about five hundred eggs produced from various crosses between three species of Gallus and their hybrids. The majority of these eggs had been fertilized, and in the majority of the fertilized eggs the embryos either had been partially developed and had then aborted, or had become nearly mature, but the young chickens had been unable to break through the shell. Of the chickens which were born,

[17] "Animals and Plants under Domestication," vol. ii., p. 189. [18] "Origin of Species," 5th edit., 1869, p. 115. [19] Ibid., p. 322.

more than four-fifths died within the first few days, or at latest weeks, 'without any obvious cause, apparently from mere inability to live,' so that from five hundred eggs only twelve chickens were reared. The early death of hybrid embryos probably occurs in like manner with plants, at least it is known that hybrids raised from very distinct species are sometimes weak and dwarfed, and perish at an early age, of which fact Max Wichura has recently given some striking cases with hybrid willows."

Mr. Darwin objects to the notion that there is any special sterility imposed to check specific intermixture and change, saying, [20] "To grant to species the special power of producing hybrids, and then to stop their further propagation by different degrees of sterility, not strictly related to the facility of the first union between their parents, seems a strange arrangement."

But this only amounts to saying that the author himself would not have so acted had he been the Creator. A "strange arrangement" must be admitted anyhow, and all who acknowledge teleology at all, must admit that the strange arrangement was designed. Mr. Darwin says, as to the sterility of species, that the cause lies exclusively in their sexual constitution; but all that need be affirmed is that sterility is brought about somehow, and it is undeniable that "crossing" *is* checked. All that is contended for is that there *is* a bar to the intermixture of *species*, but not of *breeds*; and if the conditions of the generative products are that bar, it is enough for the argument, no special kind of barring action being contended for.

He, however, attempts to account for the modification of the sexual products of species as compared with those of varieties, by the exposure of the former to more uniform conditions during longer periods of time than those to which varieties are exposed, and that as wild animals, when cap-

[20] "Origin of Species," 5th edit., 1869, p. 314.

tured, are often rendered sterile by captivity, so the influence of union with another species may produce a similar effect. It seems to the author an unwarrantable assumption that a cross with what, on the Darwinian theory, can only be a slightly-diverging descendant of a common parent, should produce an effect equal to that of captivity, and consequent change of habit, as well as considerable modification of food.

No clear case has been given by Mr. Darwin in which mongrel animals, descended from the same undoubted species, have been persistently infertile *inter se*; nor any clear case in which hybrids between animals, generally admitted to be distinct species, have been continually fertile *inter se*.

It is true that facts are brought forward tending to establish the probability of the doctrine of Pallas, that species may sometimes be rendered fertile by domestication. But even if this were true, it would be no approximation toward proving the converse, i. e., that races and varieties may become sterile when wild. And whatever may be the preference occasionally shown by certain breeds to mate with their own variety, no sterility is recorded as resulting from unions with other varieties. Indeed, Mr. Darwin remarks, [21] "With respect to sterility from the crossing of domestic races, I know of no well-ascertained case with animals. This fact (seeing the great difference in structure between some breeds of pigeons, fowls, pigs, dogs, etc.) is extraordinary when contrasted with the sterility of many closely-allied natural species when crossed."

It has been alleged that the domestic and wild guinea-pig do not breed together, but the specific identity of these forms is very problematical. Mr. A. D. Bartlett, superintendent of the Zoological Gardens, whose experience is so great, and observation so quick, believes them to be decidedly distinct species.

[21] "Animals and Plants under Domestication," vol. ii., p. 104.

Thus, then, it seems that a certain normal specific stability in species, accompanied by occasional sudden and considerable modifications, might be expected *a priori* from what we know of crystalline inorganic forms and from what we may anticipate with regard to the lowest organic ones. This presumption is strengthened by the knowledge of the increasing difficulties which beset any attempt to indefinitely intensify any race characteristics. The obstacles to this indefinite intensification, as well as to certain lines of variation in certain cases, appear to be not only external, but to depend on internal causes or an internal cause. We have seen that Mr. Darwin himself implicitly admits the principle of specific stability in asserting the singular inflexibility of the organization of the goose. We have also seen that it is not fair to conclude that all wild races can vary as much as the most variable domestic ones. It has also been shown that there are grounds for believing in a tendency to reversion generally, as it is distinctly present in certain instances. Also that specific stability is confirmed by the physiological obstacles which oppose themselves to any considerable or continued intermixture of species, while no such barriers oppose themselves to the blending of varieties. All these considerations taken together may fairly be considered as strengthening the belief that specific manifestations are relatively stable. At the same time the view advocated in this book does not depend upon, and is not identified with, any such stability. All that the author contends for is that specific manifestation takes place along certain lines, and according to law, and not in an exceedingly minute, indefinite, and fortuitous manner. Finally, he cannot but feel justified, from all that has been brought forward, in reiterating the opening assertion of this chapter that something is still to be said for the view which maintains that species are stable, at least in the intervals of their comparatively rapid successive manifestations.